

teachers themselves. . . . The admirable influence which the Scottish universities have hitherto exerted upon the people of the country has been due not only to the prolonged and systematic course of mental discipline to which their students have been subjected, but to the stimulus and encouragement given to inquiring minds by distinguished men who have made the professorial chairs centres of intellectual life; and we cannot think it desirable that any such changes should be made as would tend to lower the universities into mere preparatory schools for some central examining board."

We are gratified to think that the sentiments which we expressed in these columns nearly three years ago should have received the sanction of such high authorities. As the subject is one of great importance, our readers will perhaps allow us to repeat the objections we then raised to the establishment of a Central Examining Board (see NATURE, vol. xiv. p. 255):—

"The Calendar of the Central Board must inevitably embody only the best-known and most widely-diffused results of knowledge—not that which is growing and plastic, but that which has already grown and hardened into shape—the knowledge, in fact, of a past generation which has become sufficiently well established to be worthy of this species of canonisation. A very powerful inducement is thus offered to the professors of the various colleges to teach their pupils according to this syllabus, and a very powerful discouragement to attempt to alter it. They may be men of great originality and well qualified to extend and amend their respective spheres of knowledge, but they have no inducement to do so. . . . It is the old and time-honoured custom of killing off the righteous man of the present age in order the more effectually to garnish the sepulchres of his predecessors. Our readers are well aware that the natural philosophy course has changed its character very greatly of late years, and that for this we are much indebted to Professors Sir W. Thomson and P. Guthrie Tait. But could these men have done this under the system of a Central Board? If they had succeeded it must have been, as Galileo succeeded, against the attempt made by the ruling authorities of his day to stop his voice and strangle his originality."

It has always been a source of infinite amazement to us that a single man of eminence should come forward to advocate the gigantic apparatus of cram implied in a Central Examining Board.

May the day be far distant when the rising generation shall all be required to feed upon such rations! One is tempted to think that the advocate of this system must surely have suffered a transmutation similar to that which overtook Bottom, who, in consequence, entertained quite original notions on the subject of food. "I could munch," said that worthy, "your good dry oats. Methinks I have a great desire to a bottle of hay; good hay, sweet hay, hath no fellow."

ORGANISMS IN THE BLOOD, AND THE GERM THEORY

The Microscopic Organisms found in the Blood of Man and Animals, and their Relations to Disease. By Timothy Richards Lewis, M.B., Army Medical Department, Special Assistant to the Sanitary Commissioner with the Government of India. (Calcutta, 1879.)

WE have here in a small illustrated work an able critical *résumé* of some of the most important facts previously known on the subject together with

others not hitherto published, tending not only to increase our knowledge, but also to throw light upon the general question of the relations of the microscopic organisms found in the blood to disease.

Nearly two-thirds of the work refers to the existence in the blood of vegetal organisms of the type of Bacteria, Bacillus, and their allies, while the remaining third relates to the existence in this situation of animal organisms. We have in this latter part a brief but interesting history of what is known concerning the existence of Nematoid hæmatozoa in the lower animals, and also of what has been learned concerning the embryos of the *Filaria sanguinis-hominis*, first discovered by the author in 1872, in the blood of persons suffering from Chyluria.

It seems evident from the account here given that we have still almost everything to learn as to the source and parental forms of these embryo Nematoids found in the blood of man. The hypothesis of Manson concerning the part played by mosquitos as intermediate hosts (within which some of the embryos swallowed may undergo development, and from the bodies of which parent-forms, capable of infecting man, may find their way into drinking water) seems, from the careful observations made by Lewis, to be rendered more than doubtful. The relations of these organisms to the morbid conditions with which they are associated are, indeed, full of the most puzzling difficulties. It is somewhat doubtful whether the mature form of this helminth has yet been discovered, notwithstanding the observations of Dr. Bancroft in Australia, and of Dr. Lewis himself (as referred to on pp. 85–89). The fact of the persistence of the envelope of the ovum as a diaphanous sheath, surrounding each of the young embryos found in the blood of man, would seem to the writer strongly to suggest the probability that the embryos in question have been liberated at once into some portion of the vascular system, rather than that they have entered it from without by penetrating its walls. If such a process of struggling through tissues were to take place, their thin diaphanous envelopes would stand a good chance of being torn and left behind.

Nematoid helminths have long been known to occur in the blood of many birds, and Dr. Lewis says: "I have examined a considerable number of the ordinary Indian crow (*Corvus splendens*), and have found that the blood of nearly half of those which have come under my notice have contained embryo hæmatozoa of this character. Sometimes they are in such numbers as to make it a matter of surprise how it is possible that any animal can survive with so many thousands of such active organisms distributed throughout every tissue of its body. The birds did not appear to be affected in the slightest degree by their presence. In their movements they are very similar to the nematoid embryos found in man; they are, however, considerably smaller, and manifest no trace of an enveloping sheath."

Again, observations made many years ago by MM. Gruby and Delafond went to show that 4 to 5 per cent. of the dogs in France harboured microscopic nematodes in their blood; Lewis ascertained in 1874 that more than a third of the pariah dogs of India are similarly affected, whilst Dr. P. Manson has shown that this kind of parasitism affects at least an equal proportion of dogs in China. The embryo nematodes belonging to dogs of these

different countries seem to agree with one another in all their characters. It is important to note that their presence is not associated with the existence of any definite disease. The dogs harbouring such parasites are outwardly indistinguishable from others which have them not. Strange as this may seem, it is also strange that the mode in which the embryo organisms gain access to the blood is still involved in great obscurity. It is true that, by several observers at different times, thread-like mature nematodes (*Filaria immitis*) have been found in more or less abundance in the right chambers of the heart of the dog. These have been found to be extremely common by Dr. Manson in China, and might therefore naturally enough be considered as the source of the multitudes of embryo nematodes found in the blood of these animals. But if true for China, it ought also to hold good for India; yet Dr. Lewis says:—"It seems somewhat strange that, notwithstanding the marked prevalence of embryo hæmatozoa, the *Filaria immitis* has not, so far as I can learn, been recognised in India. I have often searched specially for it but in vain. The only mature parasite which appears to affect the circulatory system of dogs in this country is the *Filaria sanguinolenta*, a description of which, together with an account of the pathological changes which are caused by it during its development in the walls of the aorta and adjacent tissues, was published by me in 1874."¹ But then, the same writer adds:—"Notwithstanding the circumstance that this is the only mature helminth which I have found associated with the embryo hæmatozoa of India, I cannot believe that there is a genetic connection between them, for it frequently happens that the mature worm may be present in abundance unassociated with blood embryos of any kind, and sometimes it is found that the latter exist without any trace of the former."

What has been said above suffices to show the very considerable gaps in our knowledge concerning the life-history of the Nematoid hæmatozoa of man and animals, and also the tendency so frequently met with among some observers to bridge these gaps by unsatisfactory explanations deduced from a too-narrow survey of the facts—a perennial source of error peculiarly common in regard to this class of questions.

Of the protozoa referred to as being found in the blood of the lower animals the newest and perhaps the most interesting are those now first described by the author as existing in that of Rats. Being directed by the Indian Government to make observations on the spirillum occurring in the blood of patients suffering from the Bombay fever, the author says: "Whilst doing this I had occasion to examine the blood of a considerable number of animals, and eventually (July, 1877) detected organisms in the blood of a rat which, at first sight, I took to be of the nature of vibrios or spirilla." The organisms, of which figures and photographs are given, are each of them provided with a long and very distinct flagellum, though otherwise they are not very different in appearance from some bacilli. Subsequent observations showed Dr. Lewis that whilst such organisms do not seem to exist in the blood of mice they are to be found in two species of rats, viz., *Mus decumanus* and *Mus rufescens*. Concerning their prevalence and pathological significance in these animals, he

says:—"I have examined the blood of a great number of rats for the purpose of ascertaining what proportion of them contains these organisms in their blood, and find that of those specially examined for this purpose, their existence was demonstrated in 29 per cent. Sometimes, however, the numbers detected were very few, not more than one or two in a slide, but in the greater number of cases they were very numerous, every slide containing several hundreds. . . . With regard to the health of the rats in which these flagellated organisms were detected, there was nothing to suggest in any way that they were less healthy than others not so affected, and I have repeatedly kept rats for a considerable time for the purpose of observing whether any special symptoms would be manifested. . . . When it is considered that thousands of active beings of this character can exist in the blood without in any appreciable manner affecting the health of their host; and when it is further considered that these organisms must consume at least as much, if not far more, oxygen than bacteria, bacilli, and spirilla, it becomes difficult to understand how it comes about that, to a like action on the part of the latter is ascribed the asphyxia and the other morbid conditions which characterise death from splenic disease and allied affections." Such a view has been put forward by MM. Pasteur and Joubert, though it is well known, and has been pointed out by Virchow amongst others, that the proportion of bacilli in the blood at autopsies, bears no sort of relation to the severity of the disease previously existing in the persons under examination.

But it is in regard to these vegetal organisms existing in the blood of man and some animals that the larger part of Dr. Lewis's memoir refers. He evidently entertains a clear view of the principal phenomena to be considered in reference to this part of the subject, and exhibits a rare absence of a tendency, which is unfortunately but too common, to slur over fundamental difficulties standing in the way of the acceptance of the "Germ Theory of Disease"—or the "Doctrine of Contagium Vivum" as it is sometimes termed. In addition to acute criticism Dr. Lewis has made known some very significant and important new facts.

After referring to the generally received view that organisms of the bacterium or bacillus type do not exist to any recognisable extent in the blood of healthy animals, and to the experiments made some years ago by Dr. Douglas Cunningham and himself, which showed how quickly, after such organisms had been purposely introduced into the blood of healthy animals, they disappeared therefrom, he says:—"It may be safely affirmed that their presence in appreciable numbers is, judging from experience, incompatible with a state of perfect health." The case in regard to these microphytes is, therefore, different from what has been stated to obtain with the animal organisms before mentioned, which may swarm in the blood of creatures who are in other respects quite healthy.

One or other of such microphytes has been found to be generally present in *charbon* or *splenic fever*, and in *recurrent fever*. M. Pasteur has of late maintained that *septicæmia* is also characterised by the existence of such organisms in the blood during life; and to this list Dr. Klein adds the so-called *typhoid fever* of the pig.

¹ "The Pathological Significance of Nematode Hæmatozoa."

It is impossible to follow the author through his discussion (pp. 11-34) of the leading facts regarding the connection of microphytes with the diseases above mentioned, but we may briefly consider the question of their causal relation to the morbid conditions with which they are severally associated.

If the organisms of this type commonly met with outside the organism are not specifically injurious when introduced into the bodies of higher animals (and this has been abundantly proved and is commonly admitted), then, the notion that those met with in certain diseases are causes thereof, must necessarily be associated with the belief that they are organisms in some way distinct from the common forms. And this is generally the case; as Dr. Lewis says:—"All the advocates of the germ theory, with very few exceptions, maintaining that the particular organism, in the particular disease in which they are specially interested is wholly distinct from all others."

This is a position which is far from having been proved, however, and is by itself an extremely questionable doctrine. There are no real morphological characters separating the bacillus of splenic fever or of "pig typhoid" from the bacillus of hay, of urine, and of multitudes of other organic mixtures. So far as morphological characters are concerned, this is practically admitted; but then it is contended by Cohn and others that difference in "physiological property" may afford sufficient ground for the establishment of specific distinctions, even in the face of morphological similarity. This is a rather hazardous doctrine, and requires to be advanced with the greatest caution. To what extent in the vegetal and in the animal scale is it to hold good; or is it to be a distinctive character confined to the most protean and highly modifiable of all organisms? On the one hand we find such an authority as Prof. Cohn of Breslau supporting the notion; on the other a scarcely less weighty authority, Prof. Nägeli of Munich, declaring that he is unacquainted with any facts really supporting such a view. He says: "I have during the last ten years examined some thousands of different forms of fission-yeast cells,¹ but (excluding *Sarcinæ*) I could not assert that there was any necessity to separate them into even two specific kinds."

Bacilli, born and bred in the midst of the blood and tissues of a diseased animal, might have certain slight molecular differences impressed upon them, by reason of which they may tend during their nutritive life-processes to secrete a poisonous chemical principle—just as the common putrefactive bacteria are known to do—and it may thus happen that the progeny of such organic units born in morbid fluids or tissues, are capable of setting up morbid processes in the animal economy such as do not follow from the addition to it of bacilli nurtured in a bland hay infusion. This is a mere surmise, thrown out as a view which may be found by some to be easier of acceptance provisionally than the notion that, among the most variable of organisms, from a morphological point of view, several "species" present themselves under precisely the same form, and that identity or difference of "species" is to be judged by the mere effects produced by their invisible molecular activities.

Further, it should be borne in mind that the association between the organisms and the diseases in ques-

tion is not absolutely constant, nor is the severity of the disease in the least proportionate to the abundance of the organisms found in the animals affected. Speaking of recurrent fever Dr. Lewis says:—"Whereas spirilla could generally be detected in cases of fever of this kind, nevertheless cases every now and then occurred in which perfectly competent observers failed to detect them in the blood from first to last, and this too in cases not a whit less severe than those in which the organisms abounded and which were under the care of the same observers during the same period." This was the experience of Dr. Lewis himself.

Again, in regard to the same disease, the assumed cause will not operate when it is placed under the most favourable conditions—conditions in which it is scarcely conceivable that the organisms should fail to operate were they the veritable causes of the disease. Alluding to well-known experiments made by Obermier, the discoverer of the spirilla of recurrent fever, our author says:—"The inoculative experiments which he undertook, consisting of the injection of spirillum-blood of fever patients into the veins of dogs, rabbits, and guinea-pigs proved abortive, nor was there any effect produced by the injection by means of a subcutaneous syringe of small quantities of such blood into the bodies of healthy persons." Others likewise failed to reproduce the disease by similar means, though one observer states that he had been more successful in thus setting up the disease—irrespective, however, as he says, of the presence of spirilla in the blood with which inoculation was made.²

What manner of cause then is this, whose effects take place in its absence, in no corresponding ratio when present, or whose presence is followed by no effect at all? One of a strange order, truly!

But now we come to a great difficulty, an all-important matter, which in its turn has to be explained by those who cannot accept the notion that the microphytes to which we have been referring are causes of the diseases in question. Those who hold the opposite notion will naturally say to the opponents of the germ theory—But, if these organisms are not to be regarded as causes of the disease how do you account for their very frequent presence in association therewith?

Communicable or contagious diseases constitute a large class, and those in association with which microphytes have been found form only a small minority. Seeing the multitudes of observers who have been searching for them for years past in the blood of persons suffering from such affections as scarlet fever, small-pox, measles, and others, the chances that any such organisms will be found in association with these diseases may be said to be diminished to a minimum. Therefore, in so far as concerns the very frequent occurrence of organisms in the blood of persons suffering from recurrent fever, splenic fever, and some other maladies, it would be perfectly consistent (if conformable with other evidence) to regard such organisms as quasi-accidental products or epi-phenomena of the diseases in question.

If we accept the doctrine of Pasteur, Lister, and others to the effect that the blood of all healthy animals is invariably free from such microphytes, the appearance of

¹ That is, the *Schizomycetes*, in contradistinction to the true yeast-cells.

² The relations of this spirillum to other known spirilla is discussed at pp. 46-48.

organisms in the blood as epi-phenomena in the course of certain diseases can scarcely be explained except by the supposition that archebiosis or heterogenesis (one or both) have taken place in their altered blood, or in blood and tissues simultaneously.

This the present writer long ago pointed out, and he strongly insisted upon it in a paper published about eighteen months ago,¹ but which does not seem to have reached Dr. Lewis before the printing of his present work. Attention was there specially called to the fact that organisms speedily appeared in the blood of previously healthy animals or of human beings suddenly killed, in such situations and under such conditions as to make it almost impossible to account for their presence except by the occurrence of one or other of the processes above mentioned, giving rise *in situ* to a new birth of such microphytes. Organisms can, in fact, be made to appear at will (as Lewis and Cunningham, as well as Sanderson, had shown) in localised parts of previously healthy organisms by lowering the nutrition in certain ways of such parts of the body, *i.e.*, by either tying the artery supplying the part with nutrient fluid, or by subjecting the part to the influence of some germless chemical irritant. On the other hand, when the nutrient processes throughout the body are checked by the death of the animal, the production of microphytes, which was before local, now, as the writer has several times pointed out, becomes general.

Let the germ-theorists look to these facts and give us a better explanation if they can; because in the cases above referred to, organisms appear in tissues which they themselves have proclaimed to be germless, and in blood which they have declared to be free from all antecedent signs of microphytes.

The facts of the latter order have been distinctly confirmed by Dr. Lewis. He says: "Rats were obtained, killed by means of chloroform, and set aside from three to twenty-four hours or longer, according as the temperature of the atmosphere was high or low. The result proved that almost invariably bacilli were to be found in the blood, in the spleen, and in other organs."

It appears, however, and the fact is one of considerable significance, that when death takes place in certain modes (as by poisoning with carbonic acid or carbonic oxide), organisms have a still greater tendency to appear in the blood and that they manifest themselves with surprising rapidity.

A man who was sent to seek for rats, having found, "That he could procure more than could be accommodated in the cage which he had brought with him, he obtained a large earthen vessel, transferred twenty-seven rats into it, and tied a piece of cloth over the mouth of the vessel. As may be supposed, the rats had perished before he got home—all except one. . . . I examined the blood and the spleen of twenty of these rats within about six to eight hours of their having been caught, and found in each case that there were innumerable bacilli present, in every way morphologically identical with *Bacillus anthracis*.² In some of the cases the number was astonishing. They were present chiefly in the form of rods, but here and there some were seen to have grown to such

a length as to cover two fields of the microscope. . . . This experience tends to give support to the statement made by M. Signol before the French Academy, to the effect that motionless bacilli identical with those found in charbon, will be found in sixteen hours, or less, after death, in the blood of animals which have been asphyxiated by means of a charcoal fire."

Dr. Lewis shows that these organisms which make their appearance within the bodies of animals so soon after death are not only morphologically indistinguishable from *Bacillus anthracis*, but that they 'go on, under suitable conditions, to the so-called "spore" formation in precisely the same manner. The characters of these organisms under different conditions are well shown in Pl. I.

But if mere modes of dying influence the quickness with which such organisms appear in the body after death, it is not inconsistent to suppose that they may in certain cases—that is, in association with certain morbid processes—be much more prone than in others to show themselves as epi-phenomena. And this seems to correspond with what actually occurs; in many contagious diseases, as above stated, such organisms seem to be absent, in a few they show themselves, and that by far the most frequently in cases where death is already pretty closely at hand.

Referring to the bacilli met with in malignant pustule (charbon), septicæmia, and the so-called "typhoid fever," in the pig, horse, and other animals, Dr. Lewis says: "It may be confidently stated that they are never to be detected in the earlier stages of the disease, but only at a brief period before and after a fatal termination. To my knowledge they have never been found in the blood of animals which have subsequently recovered; they have always been recognised only as one of the concomitants of impending dissolution. This is undoubtedly the case so far as the two diseases first cited are concerned."

Those who are the warmest advocates of the germ-theory of disease—a doctrine resting on sufficiently unstable foundations—are not always cautious or discreet in the way they speak of others who lean to a belief that the organisms met with in association with disease are mere epi-phenomena, often produced within the body by a process of heterogenesis. Yet the latter interpretation, so far as present knowledge goes, seems to the writer essential for the explanation of our power to determine, at will, the presence of microphytes in the germless tissues or germless blood of previously healthy animals.

H. CHARLTON BASTIAN

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.]

[The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

Antiquity of Orchids

I HAVE been struck by a most cogent remark of Mr. Wallace's in his review of Mr. Allen's "The Colour Sense" (NATURE, vol. xix. p. 501), viz., "But surely in orchids the perianth

¹ "On the Conditions Favouring Fermentation, &c.," *Journal of Linnean Soc. (Zool.)*, vol. xiv. pp. 89-93.

² That is, the bacillus met with in association with charbon or splenic fever.